THE MICROSCOPE

IMPROVEMENTS IN THE LENSES OF MICROSCOPES

FOR some time, people in England have been content to let the improvement of the optical powers of the microscope remain entirely in the hands of the makers, believing, apparently, that Mr. Lister had effected all in his suggestions and improvements that could be desired. Dr. Royston Pigott, an able mathematician, formerly Fellow of St. Peter's College, Cambridge, and a Doctor of Medicine of that University, was not, however, inclined to look at the matter in this way, and for many years has been working and experimenting with a view, first, to test the accuracy of our best object-glasses, and, secondly, to suggest means for their improvement. It should be remembered that Oberhauser, Nachet, and especially Hartnack, on the Continent, not satisfied with the old system of combinations for object-glasses, and not having the benefit of Lister's researches, have made excellent objectives on a totally different system, and during the last few years the last-named maker has carried his system of "immersion lenses" to such a point of excellence as really to surpass the best glasses on Lister's system, in definition, penetration, working distance, and illumination. Those who do not admit the excellence of these objectives, which are now used by nearly all German histologists, have probably seen older glasses, made at a time when Hartnack had not reached his best. It is worth stating, now that the Parisian opticians are inaccessible, that Gundlach of Berlin has succeeded in making excellent glasses of high power at astonishingly small prices, some of his 1-12ths and 1-16ths immersion 1-16ths (so-called), being admirable in their performance. They are not, however, equal to Hartnack's glasses, which, though costing far less than what similar English glasses cost, yet are more expensive than Gundlach's. It is only fair to all parties concerned to state that the terms 1-8th, 1-12th, 1-16th, &c., as now applied to an object-glass, appear to have no definite meaning, but depend on the caprice of the maker, since the magnifying power of glasses, with the same fraction assigned to them, differs enormously.

To return to Dr. Royston Pigott. He found the usual means of testing an object-glass by trying if it gave some particular appearance with a "test object," such as the Podura-scale, very unsatisfactory, since we have no certainty to begin with as to what is the true appearance of such an object. He therefore examined minute images of objects of which he knew the true form, such as a watch-face or thermometer-scale, forming these images by aid of mercurial globules and the condenser properly adjusted below the microscope-field. By this means he has found that object-glasses corrected so as to show dark, sharply marked spines (like ! ! !) on the Podura-scale—a favourite test-object with our microscope-makers—give false, blurred, and distorted appearances with his known images, and on making such corrections of the objective as to show the known images in their true form, he finds that the Podura-scale, examined with the corrected objective, is not really marked at all, as supposed, but is beset with a series of bead-markings, which by intersection, when improperly defined, give the curious appearance like notes of exclamation. This important discovery of the falsity of our high powers (1-8th to 1-16th), has led Dr. Royston Pigott to pay more attention to the lower powers, and he finds that though you may not get so much actual amplification, you yet get a truer effect, and greater clearness of detail, by employing very carefully made low powers (1-2nd to 1-5th) and increasing the magnifying power at the other end of the microscope, *i.e.*, the eye-piece. We have in this way seen the beaded structure of the scales of Podura more satisfactorily than with very high objectives even when corrected so far as they would admit, and we may say the same of some Diatom-valves *e.g.*, *Pl. formosum*. It would be most important to know how far such a change of combination would be useful in histological work.

The general upshot of Dr. Royston Pigott's investigations appears to be that it is desirable to shift the burden hitherto cast almost wholly upon the objective, to the other parts of the instrument. We should be content with an objective as high as a fifth, or even less. A very deep eye-piece is to be used; and to correct residuary aberrations of the objective, and at the same time amplify, Dr. Pigott has introduced an important adjustable combination *between the eye-piece and the object-glass*. There seems to be considerable reason for the step proposed by Dr. Royston Pigott. Just as great results were obtained in passing from the single lens or combination to the compound microscope of eye-piece and objective, so by adding distinct integral factors to these two, such as Dr. Pigott's "aplanatic searcher," we may obtain excellences quite impossible by any amount of attention bestowed on the objective alone, or only with difficulty reached by long labour, leading to very high price for high powers.

Dr. Pigott has, during the past year, published some account of his researches in the *Quarterly Journal of Microscopical Science*, and has communicated papers to the Royal Society, one of which is about to appear in the Philosophical Transactions.

Naturally at first the makers in London and the Microscopical Society were sorely tried by Dr. Pigott's exposure of the Podura-scale, but we hear, as one good result already obtained, that Messrs. Powell and Lealand have constructed a new 1-8th both dry and immersion, with great care, which is declared to be the best glass yet made. It has been proposed to form a committee for the purpose of examining carefully as to penetration, definition, and angular aperture, the best glasses of our English makers, the best American glasses, and the best of Hartnack's, Gundlach's, and others; the glasses being mounted similarly, with private marks only for recognition, so as to prevent all possibility of prejudice on the part of the committee. Were this done, the result, whichever way it tended, would be eminently satisfactory. Of this the writer is sure, that many persons—even eminent microscopists—have made up their minds about the qualities of foreign objectives, without having seen any, or only very poor examples, and then when a really fair specimen of such a glass is placed before them, they exclaim with astonishment "Why this is the finest glass I have ever seen." We shall be glad to receive suggestions or assistance, in carrying out the proposed comparison of objectives. Dr. Royston Pigott has expressed his willingness to aid in such an undertaking.

E. R. L.

REPORT ON DEEP-SEA RESEARCHES

Carried on during the months of July, August, and September, 1870, in H.M. Surveying Ship "Porcupine" *

By W. B. CARPENTER, M.D., F.R.S., AND J. GWYN JEFFREYS, F.R.S.

THE equipment of the *Porcupine* for the previous Expedition had been found so complete and satisfactory, that nothing more was considered necessary to prepare her for the work of the present season than the overhauling of her gear, and the manufacture of new dredges, sieves, and other apparatus, on the patterns of those which had already proved most serviceable. We had the advantage of the same excellent commander, now promoted to the rank of staff-captain, with his able staff of officers; and we would take this opportunity of again expressing our deep sense of obligation to them all for their hearty co-operation in our scientific work, and for the unvarying personal kindness by which our voyage was rendered a most agreeable one. A considerable part of the crew, also, consisted of the same steady and experienced men. The Meteorological Department supplied eight of the protected Miller-Casella thermometers, including the two with the performance of which we had been so thoroughly satisfied last year; and we usually employed one of these in conjunction with one that had not been used in the previous Expedition.

At the request of the Committee, Mr. Siemens undertook to devise an apparatus for testing the depth of sea-water to which light, or at least the actinic rays, can penetrate. The foundation of the apparatus which he constructed for this purpose is a horizontal wheel with three radii, each of them carrying a glass tube in which a piece of sensitised paper is sealed up. The rotation of this wheel round a vertical axis brings each of the tubes in succession out of a dark chamber in which it ordinarily lies, exposes it to light in an uncovered space, and then carries it into darkness again. This movement is produced by a spring; but it is regulated by a detent that project from the keeper of an electro-magnet, which is made and unmadeby the completion or breaking of a circuit that connects it with a galvanic battery. When the magnet is made, it lifts the keeper with its projecting detent; and this allows the wheel to be carried by the spring through one-sixth of its rotation, whereby the first of the tubes is brought out into the open space. There it remains until the circuit is broken, whereby the magnet is unmade; the keeper then falls, and the wheel is allowed to move through another sixth of a rotation, so as to carry on the tube into the dark chamber. A repetition of the making and unmaking of the magnet brings out the second tube, and shuts it up again; and another repetition does the like with the third tube. This apparatus, with a deep-sea lead attached to it, is suspended by an insulating cable that contains the wires whereby it is connected with the battery in the vessel. Being lowered down to any desired depth, the circuit is completed, the magnet made, and one of the tubes exposed for as long a time as may be wished; the circuit is then broken, the magnet unmade, and the tube shut up again. The second tube may be exposed for a longer time in the same place, or the apparatus may be lowered to a greater depth, at which the experiment may be repeated; and the third tube may then be dealt with in like manner.—The committee having been satisfied with the performance of Mr. Siemens' apparatus, it had been arranged that trial should be made of it, and also of his Differential Thermometer, now provided with an improved Galvanometer; and he had undertaken to send out a qualified assistant to take charge of these instruments during the Mediterranean Cruise. The declaration of war between France and Germany, however, unfortunately interfered with this arrangement; the assistant (a German) being recalled to his own country, and no other competent person being available on a short notice. Under these circumstances it was thought better that the Differential Thermometer should not be sent out; but it was hoped that such a trial might be given to the Photometric Apparatus as should at any rate determine whether satisfactory results might be anticipated from its use, or whether any modifications in its construction might be needed. The apparatus was sent out to Gibraltar under charge of Dr. Carpenter, and was got into working order by his son and himself in Gibraltar Harbour. It proved, however, that the action of sea-water on the bearings,—increased as this was by the galvanic current arising out of the contact of iron and brass in them,—so embarrassed its mechanical arrangements, that no fair trial could be made of its photometric efficiency. But the experiment served the important purpose of showing the weak points of the apparatus; and neither Mr. Siemens nor Dr. Carpenter entertains any doubt that it may be so reconstructed as to answer the purpose for which it was devised.

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* Extracted from the Proceedings of the Royal Society.

The work of this year's Expedition was divided, according to the plan originally marked out, into two Cruises: the First to examine the Deep-sea bottom between Falmouth and Gibraltar; the Second to make the like examination of the western basin of the Mediterranean between Gibraltar and Malta, and to determine its Physical and Biological relations to the Atlantic,—with special reference to the Gibraltar Current. The First Cruise was under the scientific direction of Mr. Gwyn Jeffreys, who was accompanied by a young Swedish naturalist, Mr. Josua Lindahl, of the University of Lund, as Zoological Assistant; whilst Mr. W. L. Carpenter, as before, took charge of the Chemical department,—his special work, on this occasion, being the determination, by volumetric analysis, of the proportion of chlorine in samples of Atlantic water taken from the surface, the bottom, and from intermediate depths, so as to serve as a basis of comparison with similar determinations of Mediterranean water. In the Second Cruise it had been arranged that Dr. Carpenter and Prof. Wyville Thomson should co-operate as before; but the latter being unfortunately prevented by serious illness from taking part in it, the whole charge of this Cruise rested with Dr. Carpenter. He was fortunately able to retain the assistance of Mr. Lindahl; and the chemical work was continued (as in the Third Cruise last year) by Mr. P. H. Carpenter; Mr. Laughrin throughout acted as dredger and sifter.

[For the exceedingly interesting and valuable zoological details of the various dredging-hauls, we must refer our readers to the report itself.]

Throughout the whole of this cruise the temperature of the *sea-bottom* was taken by the protected Miller-Casella Thermometers in nearly every sounding. As, for the reason already mentioned, no extreme depths were sounded, and as the general rate of the diminution of temperature on the margin of the North-Atlantic basin seemed to have been established by the serial soundings taken in the expedition of the preceding year, it was not thought necessary to repeat these; more especially as the variety of depths at which the *bottom*-temperature was ascertained gave adequate data for comparison with the results then correlated. It will be shown hereafter that this comparison leads to some very interesting conclusions, fully confirming the view advanced in the last report as to the slow northward movement of an upper stratum of warm water 700 or 800 fathoms in depth, and of the southward movement of the whole deeper stratum, bringing water of an almost icy coldness from the Arctic basin into the temperate and even the intertropical zone.

During the whole of this expedition the temperature of the *surface* of the sea was ascertained and recorded every two hours, both by day and by night; as were also the readings of the Dry and Wet-bulb Thermometers, which were placed in a small penthouse on deck, in which they were freely exposed to the surrounding air, but secluded from direct or reflected solar heat. The temperature of the *surface-water*, from the time of our leaving the British Channel in lat. 48° N. to our turning the corner of Cape St. Vincent in lat. 36° 50′ N., increased at a rate which bore a pretty regular proportion to the Southing. Thus, at the "chops of the Channel," it averaged 62° for five days; whilst, by the time we approached Cape St. Vincent, it had gradually risen to above 69°. After passing that point, however, we found both the *surface* and the *bottom*-temperatures to present certain variations, which, though not considerable in themselves, proved to be of great interest when taken in connection with the peculiar condition of the *embouchure* of the Strait of Gibraltar. These points, however, will be more fitly discussed hereafter; and we shall now only notice a sudden *rise* in surface-temperature of about 3° which showed itself as we turned the corner of Cape St. Vincent and entered the *north side* of the embouchure; and a sudden *fall* of nearly 6° (to 66°·4) which was encountered when we entered the mid-stream of the narrower part of the Strait as we proceeded towards Gibraltar.

In the course of the first portion of the cruise between Falmouth and Lisbon (beyond which point Mr. W. L. Carpenter was unable to proceed), thirty-six quantitative determinations were made by volumetric analysis, of the amount of chlorine in as many samples of Atlantic water, taken (1) from the surface, (2) from the bottom at various depths, and (3) from various intermediate depths. The greater part of these, as will be shown hereafter, indicated a very close conformity to a uniform standard of density, as indicated by a specific gravity of 1·0268, and a Chlorine proportion of 11·84 per 1000;* the chief departures being observable in the *lower* density of the *deepest* waters, and in the occasional *excess* of density in the surface-waters. The former is doubtless attributable to the fact that the *deepest* water is essentially *Polar*, and therefore derives its more dilute character from that source. The latter we are inclined to attribute to the influence of slight concentration by evaporation.

Second Cruise.—Leaving Gibraltar early in the morning of Monday, Aug. 15, we steamed out into the middle of the Strait, for the purpose of commencing our experiments on the Gibraltar current. The point selected by Capt. Calver (Chart of Strait of Gibraltar, Station 39) lay midway between Point Carnero, which forms the south-eastern boundary of Gibraltar Bay, and Jebel Musa or Apes Hill, which lies opposite to it, at a distance of only 8 geographical (9¼ statute) miles, on the African coast, the Strait being here nearly at its narrowest; and it was also that at which the greatest depth (510 fathoms) was indicated by the soundings marked on the Chart. With this depth our own sounding, which gave a bottom at 517 fathoms, agreed very closely; and having thus at once found the position most advantageous for our work, that position was precisely determined by angles taken by sextant from the ship between conspicuous objects on the shore. The *bottom*-temperature obtained in the first sounding was between 5° and 6° *higher* than that which had been met with at corresponding depths on the bed of the Atlantic about 100 miles to the westward; whilst the *surface*-temperature was *lower* by from 1°·3 to 2°, as will be seen by the following comparative statement:—

	Station.	Depth.	Surface temperature.	Bottom temperature.
			°	°
Strait of Gibraltar	39	517	70·0	55·5
Atlantic	31	477	71·3	50·5
Atlantic	32	651	71·5	50·0
Atlantic	33	554	72·0	49·7
Atlantic	34	414	71·7	50·0

This striking difference led us to take a set of *serial* soundings at intervals of 50 fathoms, and these gave a result, which, though it appeared anomalous at the time, was afterwards fully explained, and proved to be of unexpected import. The temperature fell, at 50 fathoms from the surface, to 56°; at 100 fathoms it was 55°·7; at 150 it was 55°·5; and from that depth to the bottom, at 517 fathoms, there was *no further descent.* Now, it will be shown hereafter that the thermal condition, which here so much surprised us by its contrast with that of the Atlantic waters, is that universally met with in the Mediterranean, the temperature of which, whatever may be its surface-elevation, falls to within 1° Fahr. above or below 56° at a depth of 50 fathoms, to a degree lower at 100 fathoms, and then remains uniform down to the greatest depth (1,743 fathoms) at which we examined it. And it thus appears that whilst the *surface*-water in this part of the Strait is certainly derived from the Atlantic, the *deeper* water, partaking of the thermal condition which so remarkably characterises that of the Mediterranean basin, may be fairly regarded as belonging to the latter.

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* The proportion here adopted,—the number of Grammes of Chlorine to 1000 Cubic Centimetres of water, is that employed by Prof. Forchhammer in his elaborate Memoir on the Composition of Sea Water (Phil. Trans. 1865).

This inference is in harmony with another fact ascertained on the same occasion, viz., the great excess in salinity shown by water brought up from the depth of 250 fathoms over the water of the surface. Whilst the specific gravity of the latter was found to be 1·0271, that of the former was 1·0293; and whilst the proportion of chlorine in the latter was 20·034 per 1,000, it was 21·775 in the former. Now in these particulars the surface-water agreed well with what had been found to be the condition of the water of the Atlantic, whilst the water at 250 fathoms agreed equally well with what proved to be the condition of the water at the like depth in the adjacent part of the Mediterranean. We were not a little surprised, however, to find that the water taken from the *bottom* (517 fathoms) was of much *less* density, as indicated both by specific gravity and by chlorine percentage, than that of the *intermediate* stratum; its specific gravity being 1·0281, and its proportion of chlorine 21·465. This apparent anomaly (the existence of which was confirmed by observations made on our return voyage) pointed to the existence of an out-current in the *intermediate* stratum as the probable explanation of the overlaying of the lighter by the heavier water. The specific gravity of the *bottom*-stratum closely corresponded, as we subsequently found, with that of the bottom-water over the deepest part of the area of the western basin of the Mediterranean.

These data having been obtained by the examination of the several parts of the vertical column at one and the same point, and this point being in the centre nearly of the narrowest part of the Strait, and at the deepest part of the channel, we proceeded to test the *actual movement* of water on the surface and at different depths beneath it.

The rate of *surface*-movement was easily determined. The precise position of the ship having been ascertained in the manner already stated, a small flat basket, presenting no such elevation above the water as would cause it to be influenced in any considerable degree by a moderate wind,* was sent adrift, so as to be freely carried along by the current; it was allowed to float for a determinate time, throughout which it was followed by the ship, and when it was taken up at the expiration of that time, the place of the ship was again ascertained as before. The space between the two points being then determined trigonometrically, the rate of the flow per hour, and its precise direction, could be readily calculated. Thus on the morning of August 15th the float was followed by the ship for fifteen minutes, during which it was found to have moved 4,377 feet in the direction E. by S. ¾ S., or at the rate of 2·88 miles per hour.

For the determination of the *movement of the water at different depths below the surface*, a current-drag had been constructed by Capt. Calver on a plan suggested by his previous experience; which had led him to the conclusion that a submerged basket lined with sail-cloth, which of course fills itself with water, presented a better resisting surface than any vessel of wood or metal. Such a basket being made the basis (so to speak) of the apparatus, its resisting surface was augmented by fixing two pairs of arms at right angles to one another across its upper end, and stretching a piece of sail-cloth between each arm and the side of the basket, which device caused a uniform resisting surface to be presented to the current whatever the manner in which the sails might meet it. To the lower part of this "drag" a couple of sinkers, of 112lb. each, were attached; and the whole apparatus was supported by cords meeting in a ring above it, to which the suspending line was secured.

This "current-drag" having been transferred to a boat, was lowered down by a couple of men placed in her, to the desired depth; and the boat was then left entirely free to move, being lightened by the return of the men into the ship. The motion of the boat would be the composite result of (1) the action of wind (if any) upon the tranverse section of the part of the boat above the water; (2) the action of the surface-current upon the transverse section of the immersed part of the boat; (3) the action of the upper current upon the suspending line; and (4) the action of the current in which the "drag" is suspended, upon the drag itself. Putting aside the *first* of these agencies, which will be of very little account if (as in the experiment now narrated) the boat be small and the breeze be light, it is obvious that the relative influence of the *second* and *third* to that of the *fourth* will depend upon the proportion between the surfaces presented by the boat, the line, and the "drag" respectively, and the strength of the current acting upon each. The surface given to the "drag" being larger than that of the boat and line taken together, the force acting on the "drag" will dominate, if it hang in an opposing current superior, equal, or even somewhat inferior in rate to that which acts on the boat and line, so that the boat would be carried along by the drag *against* the surface stream at a rate proportioned to the excess. If, again, the rate of the under-current should be greatly inferior to that of the surface, its action upon the "drag" might still be sufficient to neutralise that of the surface-current upon the boat and line, and the boat would then remain *stationary*, or nearly so. A still further reduction in the rate of the opposing under-current would make its action upon the "drag *less* powerful than that of the surface-current upon the boat and suspending-line, and the boat would then move *with* the surface-current but at a rate of which the great retardation would indicate an antagonistic force beneath. Supposing again, the water of the stratum in which the "drag" is suspended to be stationary, the action of the surface-current upon the boat and line would be opposed by the resistance offered by the deeper water to the movement of the drag; and the retardation of the movement of the boat would be less, though still considerable.—If, again, the stratum in which the "drag" is suspended should itself be moving in the direction of the surface-current, but at a reduced rate, there will still be a resistance to the movement of the "drag" at the more rapid rate of the surface-current; and this resistance will produce a proportional retardation in the motion of the boat. Finally, if the stratum in which the "drag" is suspended, with the intermediate stratum through which the suspending line passes, move at the same rate with the surface-current, the motion of the boat with the whole suspended apparatus will have the same rate as that of the simple float.

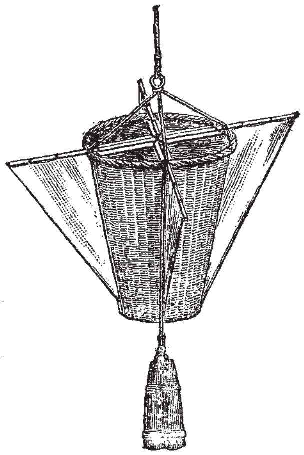

FIG. 1—CURRENT DRAG

Putting these respective cases conversely, it may be affirmed (1) that if the boat, having the "current-drag" suspended from it, should move *with* the surface-current and *at the same rate*, the stratum in which the "drag" hangs may be presumed to have a motion nearly corresponding with that of the surface-current; (2) that if the rate of movement of the boat *with* the surface-current should be *retarded*, a diminution of the rate of the stratum in which the drag hangs to a degree *exceeding* the retardation of the movement of the boat, may be safely predicated; (3) that when this retardation is so considerable that the boat moves *very slowly* in the direction of the surface-current, it may be inferred that the stratum in which the drag is suspended is either stationary, or has a slow movement in the opposite direction; (4) that if the boat should remain *stationary*, a force must be acting on the "drag" which is equal, and in the *contrary direction*, to that of the upper current upon the boat and suspending line; so that the existence of a counter-current is indicated, having a rate as much *less* than that of the surface-current as the resisting surface presented by the "drag" is greater than that offered by the boat and upper part of the suspending line; (5) that if the boat should move in a direction *opposed* to that of the surface-current, a motion is indicated in the stratum in which the "drag" hangs, which will correspond in *direction* with that of the boat, and which will *exceed* it in *rate*, the effect of the "drag" upon the boat being partly neutralised by the antagonistic drift of the surface-current.

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* It is obvious that the movement of the ship itself would be liable to be considerably affected by even a slight breeze, on account of the large surface of resistance presented by its transverse section (especially by its paddle-boxes) above the water. This would cause its drift to be *more* rapid than the current, if the direction of the wind should be *with* that of the current, and *less* rapid if the wind should be opposed to it.

Now our first set of experiments with the "current-drag" gave the following results:—

(1.) The surface-movement being first tested in the manner already described, its rate was found to be 2·88 nautical miles per hour, and its direction E. by S. $\frac{3}{4}$ S. The wind was W. by N., with a force of 4.

(2.) The "drag" having been lowered down to a depth of 100 fathoms, the rate of movement of the boat from which it was suspended was reduced to 1·550 mile per hour, or *rather more than half* the surface-movement. Its direction was E. $\frac{1}{3}$ S. Taking into account the action of the wind and surface-current on the boat, it may be safely affirmed that at 100 fathoms the rate of the current was reduced to *less than one half.*

(3.) The drag having been lowered down to a depth of 250 fathoms, the boat remained nearly stationary, its *rate* of movement being reduced to 0·175 mile per hour, while its direction (S.E. $\frac{1}{4}$ E.) was slightly altered to the southward, though still easterly. From this we felt ourselves justified in inferring that the 250-fathoms' stratum had a movement in the *reverse direction*, acting on the current-drag with a force almost sufficient to neutralise the action of the upper stratum on the boat and suspending line. And this inference is strengthened by the extraordinary density of the water of this stratum, and is fully justified by the results of the experiments which we made on our return-voyage.

While these experiments were in progress, we had the pleasure of seeing the Channel Fleet, which was expected to meet the Mediterranean Fleet at Gibraltar, come in sight beyond Cape Tarifa; its approach having been indicated, long before even the tops of the masts of the vessels composing it showed themselves above the horizon, by the number of separate puffs of smoke which the experienced eye of our Captain enabled him to distinguish. As soon as all possibility of doubt was removed by the appearance of the masts, Capt. Calver communicated "Fleet in sight" by signals to the Admiral in Gibraltar Harbour; our position being such that *we* could be seen by him, though the Fleet could not. In due time, the massive hulls of the ironclads rose above the horizon, and whilst we continued at our work, all passed us in sailing order, at a distance of not more than a couple of miles, the ill-fated *Captain* being the chief object of interest. A few hours later, the *Monarch*, which had been detained for repair, but whose passage had been made in a shorter time by the free use of her steam-power, came in sight, and passed on in solitary grandeur to join the fleet, now united in Gibraltar Bay.

The whole of our first day having been consumed without our being able to work the "current-drag" in the deepest stratum, we anchored for the night near Point Carnero, with a view to resuming our experiments on the following morning. We then ran out to a spot almost precisely identical with that which had been our starting-point on the previous day; and commenced as before, by testing the rate and direction of the surface-movement. Its rate proved rather slower, being 2·40 miles per hour, instead of 2·88; and its direction was E. by N., instead of E. by S. $\frac{3}{4}$ S. Both differences seemed to be accounted for by the difference in the force and direction of the wind; which, having been W. by N. with a force of 4 on the previous day, was now W. $\frac{1}{2}$ S. with a force of only 2. The "drag" was then lowered to a depth of 400 fathoms; but our expectation that it would there encounter a westerly (or outward) current sufficiently strong to carry the boat in that direction in spite of the antagonistic movement of the easterly (or inward) surface-current, was not verified on this occasion; for the boat slowly drifted in an E. $\frac{1}{3}$ N. direction, its rate being 0·650 mile per hour. Whether this result should be taken to indicate a stationary condition of the deep stratum, or a slight movement in either direction, can scarcely be affirmed with positiveness; but from the indication afforded by the specific gravity of the water taken up from this depth, it would seem probable that the general movement of this stratum was at this time rather *westerly*, or in conformity with that which we attributed to the intermediate stratum, though at a slower rate. It will be shown hereafter that a decisive proof of such a movement was obtained on a subsequent occasion.

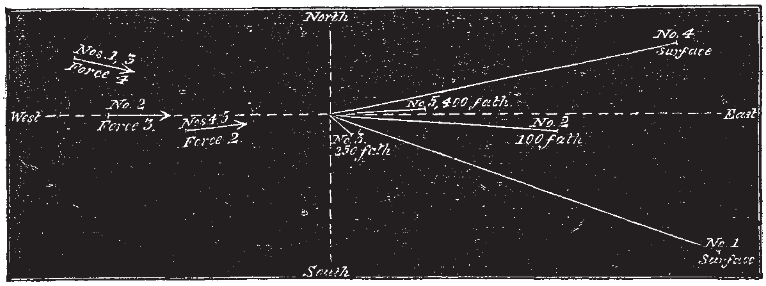

FIG. 2.—Rate (per hour) and Direction of Movement of Surface-Float, and of Current-Drag at different Depths; with Force and Direction of Wind

Thinking it expedient to postpone the further prosecution of this inquiry until our return-voyage,—when we should be able to repeat our experiments, not only at this narrow end of the Strait where it enters the Mediterranean Basin, but also at that shallowest portion to the westward, where the Strait opens out into the Atlantic,—we put steam on before mid-day, and entered the basin of the Mediterranean, directing our course in the first instance to the spot (lat. 36° 0′ N., long. 4° 40′ W.) at which the sample of bottom-water had been obtained by Admiral Smyth, which, when analysed by Dr. Wollaston, was found to possess the extraordinary specific gravity of 11·288, and to yield a percentage of 17·3 of salt.* As we were within sight of both shores, and could distinguish several remarkable mountain-summits which were accurately laid down on our charts, the bearings of these enabled the situation of the ship to be determined with great precision; and Captain Calver undertook to place her within a mile of the point at which Admiral Smyth's observation had been taken. Having reached this, we took our first sounding in the Mediterranean, and awaited the result with no little interest. The depth proved to be 586 fathoms, or 84 fathoms less than that given by Admiral Smyth's sounding; but as the latter was not taken on the improved method now adopted, and as its correctness may have not improbably been affected by the strength of the easterly current which is here very perceptible, the discrepancy can scarcely be considered as of any real account as showing that the two points were otherwise than nearly coincident.† The

* Phil. Trans. for 1829, p. 29; and Admiral Smyth's "Mediterranean," pp. 128—130.

† Thus Admiral Smyth states ("Mediterranean," p. 159) the depth in mid-channel between Gibraltar and Ceuta to be 950 fathoms; whereas it is now known to be but little more than 500 fathoms. "A little farther to the eastward," he says, "there is no bottom with 1,000 fathoms of line up-and-down (*upwards of* 1,300 *payed out*);" whereas the greatest depth as far east as Malaga Bay is now known not to exceed 750 fathoms. These errors are noticed in no invidious spirit, but merely to prevent their perpetuation. Admiral Smyth doubtless made the very best use of the means at his disposal; but a far more satisfactory method has now entirely superseded that formerly adopted.

specimen of bottom-water brought up by our bottle was found to have a specific gravity of 1·0292, whilst that of the surface-water was 1·0270. The volumetric determination of the chlorine gave 21·419 per 1,000 for the bottom-water, as against 20·290 per 1,000 for the surface-water. A decided excess of salt is thus indicated in the bottom-water, as compared on the one hand with the surface-water of the same spot, and on the other, with the bottom-water of the Atlantic, which had been generally found to show a rather smaller proportion of chlorine than the surface-water. But this excess is extremely small in comparison with that indicated by Dr. Wollaston's analysis. For, assuming his factor of ·134 as representing, when multiplied by the excess of specific gravity above that of distilled water, the total per-centage of salt, that per-centage is only 3·91, instead of being 17·3, as stated by Dr. Wollaston.

This result accorded so closely with that obtained by Dr. Wollaston himself from the analysis of two other samples of bottom-water taken up by Admiral Smyth, the one in long. 1° E. from a depth of 400 fathoms, and the other in long. 4° 30′ E. from a depth of 450 fathoms, as well as with our own determinations of the specific gravities and chlorine per-centages of a great number of samples taken in different parts of the Western basin of the Mediterranean, that we cannot hesitate in regarding it as representing the *ordinary* condition of the bottom-water at this spot. And it seems to us far more probable that the sample furnished by Admiral Smyth to Dr. Wollaston had been concentrated by evaporation in a badly-stopped bottle in the three years during which it had remained in Admiral Smyth's possession, than that any extraordinary discharge of salt from a brine-spring at the bottom (a sort of *Deus ex machinâ* invoked by Admiral Smyth to account for the occurrence) should have given rise, in the spot at which his sounding was taken, to an exceptional condition of which no indication whatever was presented in our own.

The temperature-phenomena presented at this Station proved of singular interest. The *surface*-temperature, 74°·5, was higher than any that had been encountered on the Atlantic side of the Straits, even in a latitude half a degree farther south; and the observations which had been regularly taken every two hours showed that it had increased nearly *ten degrees* as we proceeded eastwards from Station 39, between 10 A.M. and 2 P.M. A part of this increase was doubtless due to the heating effect of the mid-day sun; but as the temperature of the air had not increased quite *six* degrees during the same time, and as it will be shown hereafter by a comparison of the diurnal averages of the surface temperature of the Mediterranean with those of the Atlantic, that the latter are at least four or five degrees higher than the former, it may be fairly assumed that at least half the increase was due to the passage from the colder Atlantic water of the mid-channel into the warmer water of the Mediterranean basin, the temperature of the latter being even here somewhat reduced by the inflow of the former.—The *bottom*-temperature was found to be here 55°; and this corresponded closely with that which we had met with in the Strait, while it was at least 5° higher than had been obtained at corresponding depths on the Atlantic side. Being desirous of determining the rate of its diminution, we took *serial* soundings at intervals of 10 fathoms down to 50, and then at 100 fathoms; with the following remarkable result:—

		°		°
Surface		74·5	diff.	5·2
10 fathoms		69·3	diff.	4·3
20 ,,		65·0	diff.	2·0
30 ,,		63·0	diff.	1·3
40 ,,		61·7	diff.	2·0
50 ,,		59·7	diff.	4·6
100 ,,		55·1	diff.	0·1
586 ,,	(bottom)	55·0		

Thus there was a fall of 9°·5 in the first 20 fathoms; of 5°·3 in the next 30 fathoms; of 4°·6 in the next 50 fathoms; whilst from 100 fathoms to the bottom at 586 fathoms there was no further descent.

Whilst we were prosecuting these inquiries, we found ourselves surrounded—the surface of the sea being extremely calm—by great numbers of the beautiful floating *Velellæ*, which are occasional visitors to our own coast, accompanied by the *Porpitæ*, which are more exclusively restricted to warmer seas. With these was a great abundance of a small species of *Firola*, about 0·4 inch in length, the extreme transparence of which enabled every part of its organisation to be readily studied microscopically, its nervous system being specially distinguishable. Of this very interesting *Heteropod*, a full description will be hereafter published by Dr. Carpenter.

The result obtained by our first temperature-sounding in the Mediterranean, was fully borne out by that of the temperature-soundings taken during three subsequent days; which show an extraordinary uniformity of *bottom*-temperature at depths from 162 to 845 fathoms*:—

Station. No.	Depth, in faths.	Surface-temp.	Bottom-temp.
		°	°
41	730	74·5	55·0
42	790	74·0	54·0
43	162	74·7	55·0
44	455	70·0	55·0
45	207	72·7	54·7
46	493	73·5	55·5
47	845	69·5	54·7

It will be observed that the surface temperature varied between 69°·5 and 74°·5; and that whilst the highest temperatures were shown at Stations near the African coast, the lowest presented itself between Cape de Gat and Cartagena. Now the Gibraltar inflow is very sensibly felt at Cape de Gat, where the current usually runs at the rate of a mile an hour; and of the strength of this current we had unpleasant experience. For on the 19th of August, as we were crossing from Station 46 towards the Spanish coast, we encountered a strong N.E. breeze, which, meeting the current, worked up a considerable swell; this prevented us from taking even a temperature-sounding on that day, and gave our ship a peculiar twisting or screwing movement, from which we were glad to escape by the subsidence of the breeze during the following night. During this day the surface-temperature of the sea came down from the average of 72°·2 which it had maintained on the 18th, to 66°·9. Had the weather been calm, we might have attributed this reduction to the colder Gibraltar in current; but as the average temperature of the air also fell from 73°·8 to 69°·8, and as the strong N.E. breeze must have had a cooling effect upon the surface of the sea, we should have deemed it probable that the reduction of surface-temperature was due at least as much to the latter as to the former of these causes, had it not been that a set of serial soundings which we took at Station 47 showed that the reduction extended very far down, as will be apparent on comparing the following results with those previously given:—

	°Fahr.
Surface	69·5
10 fathoms	59·0
20 ,,	57·5
30 ,,	56·5
40 ,,	55·7
50 ,,	55·3
100 ,,	54·7
845 ,, (bottom)	54·7

It will be seen hereafter that the observations made on our return voyage gave more distinct evidence of the cooling influence of the Gibraltar in-current.

At most of these stations we explored the bottom by means of the dredge, with results much less profitable than we had anticipated. Except near the coast, on either side, where the ground was rocky and unequal, the bottom was found everywhere to consist of a tenacious mud, composed of a very fine yellowish sand mixed with a bluish clay—the former predominating in some spots, the latter in others. Large quantities of this mud were laboriously sifted, often without yielding anything save a few fragments of shells, or a small number of *Foraminifera*; and in no instance was it found to contain any considerable number of living animals of any description. Our disappointment at this unexpected paucity of life was not small; and it was destined, as will hereafter appear, to continue through the whole of our dredging exploration of the deeper portions of the Mediterranean basin. The operation of dredging in the shallower portions nearer shore was rendered difficult by the rocky nature of the bottom, on which the dredge continually "fouled;" and after the loss of two more dredges and a considerable quantity of rope, Capt. Calver came to the conclusion that the "tangles" only should be used where the inequality of the soundings indicated danger to the dredge. Now the "tangles," whilst gathering

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* This uniformity, as we have since learned, had been previously observed by Captain Spratt, in his Soundings in the Eastern Basin of the Mediterranean; but owing it seems probable to the want of "protection" in his thermometers, he had set the uniform temperature too high, namely 59°. (See his "Travels and Researches in Crete," vol. ii. Appendix II.)

Polyzoa, Echinoderms, Crustacea, and the smaller corals, sometimes even better than the dredge, pick up but few shells; and hence our collection of Mollusca is altogether a scanty one. Nevertheless, many of the types we did obtain were of considerable interest. Thus at Station 45, at a depth of 207 fathoms, we got *Turbo Romettensis*, Seguenza, MS. (Sicilian fossil); *Scalaria plicosa* (Sic. foss.); *Odostomia obliquata*, Ph.; *Philine*, two undescribed species; and an interesting coral (*Dendrophyllia corrugosa*).

SCIENTIFIC SERIALS

THE *Geological Magazine* for January (No. 79) commences with an article by Mr. A. H. Green, on a subject which has lately attracted much attention, namely, the "Geological Bearings of the recent Deep-Sea Soundings." Mr. R. Tate contributes an article on the Invertebrate Fossils of the Lias, which includes a useful table of the known Liassic genera, showing the number of species in each found in Britain and on the Continent, with observations on the distribution of the genera, and descriptions of some new species. Mr. J. Clifton Ward has a paper on the Development of Land, illustrated by references to the geological history of Italy and England. In an article on the Transport of Wastdale Crag Blocks, Mr. Croll ascribes the distribution of those puzzling boulders to the action of land-ice, and adduces evidence in support of the opinion that the great ice-covering of Scotland overlapped the high grounds of the North of England. Mr. Hopkinson characterises a new genus of Graptolites, under the name of *Dicellograpsus*; it includes five species hitherto referred to *Didymograpsus*.

The February number of the same journal opens with a note on the Diamond-fields of South Africa, by Prof. T. Rupert Jones, in which the author endeavours to correlate the scattered and scanty information that we possess on the geology of that region. MM. Brady and Crosskey furnish lists of fossil Ostracoda obtained from the post-tertiary deposits of various localities in Canada and New England, with descriptions of six new species, illustrated by an excellent plate. From the Rev. Osmond Fisher, we have some interesting notes on phenomena connected with denudation observed in the so-called coprolite pits near Haslingfield in Cambridgeshire; and from Mr. Woodward a note on the new British Cystidean, *Placocystites Forbesianus* of De Koninck, which is identified, on the authority of Mr. Billings, with his *Ateleocystites Huxleyi*. The numbers also contain the usual notices, reviews, and smaller communications.

OF the *Württembergische Naturwissenschaftliche Jahreshefte* we have received the twenty-sixth volume, published in 1870, and including, besides the ordinary general notices of the Proceedings of the Natural History Society of Württemberg, some important papers in various departments of science.—Dr. Samuel Bartsch contributes a notice of the Rotifera observed by him in the neighbourhood of Tübingen, which he preludes with remarks on the anatomy and physiology of those animals. The species are not described, but the genera are characterised, and the author contributes valuable remarks on some of them. He establishes two new families: the Longisetæ, for the genera *Distemma*, *Rattulus*, *Furcularia*, and *Monocerca* of authors, and a new genus, *Monommata*, including *Notommata tigris* and *longiseta*; and the Loricata, for the reception of Ehrenberg's *Euchlanidota* and *Brachionæa*.—Prof. Fraas describes the progress of the geological investigation of Württemburg, and M. C. Deffner, the very curious structure of the Buchberg, an outlier of the Jura, near Bopfingen.—The most important geological paper is a notice of the fauna of Steinheim, relating principally to the remains of Miocene mammals and birds from that locality. This fauna includes, together with numerous well-known species some new forms of great interest, among which may be noticed especially, a species of *Colobus*, described by the author as *C. grandævus*; a new generic type allied to the badgers, and described as *Trochotherium cyamoides*; and species of *Chæropotamus* and *Tapirus*. The fauna is considered by the author to have its nearest existing representative in South-Eastern Asia and the great islands of the Indian Archipelago, and he regards the deposit as following in order of time, the "Langhian stage" of Carl Mayer, which includes the deposits of Weissenau, Oppenheim, Radoboj, and others in Germany; and of Arquato, Superga, Malta, and others in the South of Europe. This valuable memoir is illustrated with six plates.—Prof. C. W. Baur reports on recent geodetic surveys made in Württemberg, for the purpose of a European measurement of a degree.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 9.—"On the Problem of the In-and-Circumscribed Triangle," by A. Cayley, F.R.S.—The problem of the in-and-circumscribed triangle is a particular case of that of the in-and-circumscribed polygon; the last-mentioned problem may be thus stated—to find a polygon such that the angles are situate in and the sides touch a given curve or curves. And we may in the first instance inquire as to the number of such polygons. In the case where the curves containing the angles and touched by the sides respectively are all of them distinct curves, the number of polygons is obtained very easily and has a simple expression; it is equal to twice the product of the *orders* of the curves containing the several angles respectively into the product of the *classes* of the curves touched by the several sides respectively, or say it is equal to twice the product of the orders of the angle-curves into the product of the classes of the side-curves. But when several of the curves become one and the same curve, and in particular when the angles are all of them situate in and the sides all touch one and the same curve, it is a much more difficult problem to find the number of polygons. The solution of this problem when the polygon is a triangle, and for all the different relations of identity between the different curves, is the object of the present memoir, which is accordingly entitled "On the Problem of the In-and-Circumscribed Triangles;" the methods and principles are, however, applicable to the case of a polygon of any number of sides, the method chiefly made use of being that furnished by the theory of correspondence.

Anthropological Institute of Great Britain and Ireland, February 14.—Sir John Lubbock, Bart., M.P., F.R.S., President, in the chair. The President stated that this was the first meeting of the "Anthropological Institute of Great Britain and Ireland," since the union of the late Ethnological and Anthropological Societies under that name, and having vacated the chair in favour of Professor Huxley, proceeded to read a paper, "On the Development of Relationships." After some preliminary observations on the character of the family among the lower races of men, and the preponderance of the tribal tie, Sir John proceeded to discuss the conclusions drawn by Mr. Morgan from the valuable schedules of relationships collected by him and published by the Smithsonian Institution, especially with reference to his theory that the similarity between the Mohawk and Tamil systems indicated any Ethnological affinity between those races, a conclusion which Sir John was unable to accept. He then proceeded to show how in his opinion that similarity had arisen, and traced up the gradual development of correct ideas on the subject of relationships from the system of the Sandwich Islanders, which is the lowest on record, step by step to that of the Karens; showing that in each system there are points which can only be explained on the hypothesis of its development from a still ruder condition. He then compared these actually existing systems with those which would be produced by a retrogression of social customs, and showed that the systems of the lower races all indicate progress, and there are no instances of the existence of such a system as would arise in the case of degradation. He also laid stress on the fact that the social system is invariably in advance of the nomenclature of relationships, another evidence of progress as opposed to degradation. He showed that even in some European nations we have traces of earlier lower condition, and that therefore in the systems of relationships we have an interesting proof of the social progress of man, and the gradual development of family ties.—Mr. Hodder M. Westropp exhibited a Worked Flint Implement, said to have been found in a barrow, Ashby Down, near Ventor, Isle of Wight.—The Chairman announced that the meetings of the Institute would, during the remainder of the Session, be held on Mondays, commencing on Monday, 6th March.

Chemical Society, February 16.—Prof. Williamson, F.R.S., President, in the chair. The following gentlemen were elected fellows:—W. D. Herman, W. W Houlder, G. Lockyer, jun., W. J. Lockyer, J. E. Mayer, R. Meldola, M. M. P. Muir, W. J. Reynolds, W. Smith, T. E. Thorpe. The following papers were read:—"On the production of wood spirit," by E. T. Chapman. The author began by remarking that the difficulties of obtaining correct information regarding this production are very great. The manufacturers are too jealous to disclose their